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SOME IMPLICATIONS OF ANTI-INTELLECTUALISM

THE sea-change that unconsciously comes over philosophic terms is well illustrated in the shift of meaning that the term "intellectualism" is even now undergoing. In its traditional sense, the word is used to denote the antithesis to the sensational theory of knowledge, viz., the theory that *intellectus ipse* furnishes the indispensable conditions or ingredients of knowledge. At present it is frequently used to contrast with voluntarism. It denotes what appears to its opponents to be a false estimate of the place of knowledge in experience; and an exaggeration of the value, *in philosophy,* of strictly logical criteria and methods. From this point of view, sensationalism itself is a form of intellectualism, in so far as it starts with sensations as cognitive units, and treats appreciation and effort as derivatives of such terms, or as accretions of them.

It is easy, however, to overstate the extent to which this shift of meaning has been accomplished. A flavor of the earlier controversy still hangs about the current polemic against intellectualism. Often it is not apparent whether anti-intellectualism is urged as a protest against the false placing of the knowledge point of view (irrespective of how knowledge is constituted), or as a protest against emphasizing rational considerations in knowledge at the expense of the empirical. This ambiguity is the more natural because the two manners of anti-intellectualism have much in common and readily run together. Nevertheless, confusion is likely to result from failure to discriminate the two types; accordingly I introduce my discussion with an accentuation of their contrast.

One form of intellectualism may remain at the knowledge point of view as ultimate. It may hold that knowledge is the true access to existence, and that "things *are* what they are *known* to be." But it upholds the superior claims of non-rational, non-logical factors in constituting the web of things known. It especially antagonizes the claim to supremacy in knowledge of rationalizing functions, of conceptions, of abstract and general principles, laying the stress upon

the particulars, the uniques, the givens, which resist reduction to thought. Since the rationalist is led by the undeniable presence of such irrational surds in *our* knowledge to the ideal of a higher reality which is the content of valid (*i. e.*, rational) knowing, this anti-intellectualism also contends against the doctrine of another (noumenal and absolute) reality behind the world of things concretely known.

With the spirit of this polemic, the other sort of anti-intellectualist is in full sympathy. But he introduces two differences of emphasis. In the first place, he may attach considerably more value to logical functions in constituting knowledge than does his fellow-in-arms. He may hold that knowledge is what it distinctively is precisely by the operation of factors of mediation, that is, of abstraction, generalization and logical relationship. He may hold that concepts render things more intelligible, not less so; even that they are the *only* means of rendering things *intelligible*. But, in the second place, he will hold that rendering things intelligible is a special and peculiar function, having its own special origin and aim; and in making it his business to find out what is the specific character of knowledge he finds that "making things intelligible" is, in origin and in effect, a function operated in the interests of behavior. Accordingly, what he objects to in the intellectualist is not so much the latter's emphasis upon logical factors in knowledge, as his isolation of the knowledge standpoint (in procedure and criteria), from its functional place and rôle—an isolation which is equivalent, of course, to making knowledge an ultimate and all-inclusive philosophic criterion.

If we call both types of anti-intellectualism by the name of pragmatism, certain significant differences present themselves in pragmatic theory. The form of pragmatism which remains at the standpoint of knowledge (insisting upon the presence of non-rational factors in the knowledge structure) is akin historically to British nominalism and sensationalism—save that it breaks loose from the isolated and atomic character assigned by British empiricism to "sensations," assimilating the latter rather to the continuity and flow of vital feeling. In its insistence upon testing conceptions by consequences, it holds that the important thing is that the consequences shall be particular (*i. e.*, concretely experienceable) rather than that they shall be active.

The other type of pragmatic anti-intellectualism starts from acts, functions, as primary data, functions both biological and social in character; from organic responses, adjustments. It treats the knowledge standpoint, in all its patterns, structures, and purposes, as evolving out of, and operating in the interests of, the guidance and enrichment of these primary functions. The vice of intellectualism

from this standpoint is not in making of logical relations and functions in and for knowledge, but in a false abstraction of knowledge (and the logical) from its working context. It may be proper to add that instrumental pragmatism (or the doctrine of what Mr. James has called the "Chicago School") is distinctively of this last type. Bearing this fact in mind might perhaps clarify some of the obscurities found in this branch of pragmatism.

These things being premised, I want to say a few words, by way of *obiter dicta,* upon the implications of this latter type of anti-intellectualism. In the first place, as to its bearings upon the proper way of conceiving philosophy. Philosophy is itself a mode of knowing, and of knowing wherein reflective thinking is much in play. It is hence self-contradictory for an instrumental pragmatism to set up claims to supplying a metaphysics or ontology. As a mode of knowledge, it arises, like any intellectual undertaking, out of certain typical perplexities and conflicts of behavior, and its purpose is to help straighten these out. Philosophy may indeed render things more intelligible or give greater insight into existence; but these considerations are subject to the final criterion of what it means to acquire insight and to make things intelligible, *i. e.,* namely, service of *special* purposes in behavior, and limit by the *special* problems in which the need of insight arises. This is not to say that instrumentalism is merely a methodology or an epistemology preliminary to more ultimate philosophic or metaphysical inquiries, for it involves the doctrine that the origin, structure, and purpose of knowing are such as to render nugatory any wholesale inquiries into the nature of Being.

This conclusion leads to my next remark. Wherever the intellectualistic bias in philosophy operates there is inevitably an attempt to discriminate between what is real being and what is not, between reality and appearance, between objective existence and the subjective—which somehow exists and yet has no true existence. From the instrumental standpoint this extraordinary performance (so extraordinary that it is usually taken as the defining mark of philosophy) is easily explicable. It is the business of any knowledge event to discriminate, *in a given specific situation of perplexity and consequent indeterminate response,* between what is genuine and what is counterfeit, the veridical and the illusory, what is and what merely seems, what is valid or objective and what is invalid or "subjective." If we have regard for the proper setting and limitations of the knowledge event, it is obvious that these distinctions are wholly relevant and relative to *making an inference or drawing a conclusion,* which in turn is a condition of projecting, auspiciously, further behavior. If we are committed to the intellectualistic fallacy, we forget these limitations of context and purpose; we take this type of discrimina-

tion wholesale and transfer its terms to being or existence in general. Since plainly no common form and no scientific form of knowledge indulges in such preposterous extension beyond the control of a specific situation, "philosophy" comes to be a uniquely mysterious discipline. The universe at large being divided into the real and the unreal, the objective and the subjective, being and appearance, absolute and phenomenal, gives rise inevitably to all sorts of problems as to how these antithetical things get on together; there being none of the specific tests that condition common-sense knowing and science, these inquiries may proceed indefinitely.

Another problem which gets placed in a very different light when the intellectualistic fallacy is avoided is that which Perry has recently named in these pages as the *ego-centric predicament*.[1] From the intellectualistic point of view, the self that is implicated in every knowledge event has to be conceived as a term of the knowledge relation; the intellectual function being final and inclusive, there is no other way of disposing of it. Hence the self, the ego, the subject, is at once identified with "mind" or "consciousness" (or whatever), and the latter is treated as one of the two correlative constituents of knowledge, the object known being the other. Then the whole brood of "epistemological" problems swarms. Sticking to the facts of empirical situations, the ego, subject, self is seen, however, to be simply the agent that undertakes and is responsible for the cognitive event. The relation in question is that of an agent to its act, not that of one of the two terms of knowledge to the other term. Difficulties may be attached to the proper conceiving of the relations of agent and act; but at all events they are specific, concrete difficulties of the same sort that manifest themselves in the consideration of any function of any living organism. They do not concern a relation constitutive of a special discipline, called epistemology, a relation found nowhere outside of the epistemology that deals with it. In other words, the wholesale "ego-centric predicament" disappears, and for it is substituted the concrete question of how an act in the way of knowing is related to other types of action.

I am quite aware of the dogmatic and seemingly arbitrary sound of these utterances. Their purpose, however, is not to prove anything, but simply to outline, with some emphasis, the implications of a certain type of anti-intellectualism. There is nothing specially novel in the anti-intellectualism that accepts the stock problems of the intellectualist philosopher, and then attempts to solve them by calling in the assistance of non-intellectual factors, such as feeling, immediate intuition, faith or volition; this is a standing device of the whole history of philosophy. There is something more promising in

[1] This JOURNAL, Vol. VII., p. 5.

an attempt which, accepting the complete right and autonomy of knowing and of logic in its own field, tries to see what this field of knowledge and reflective intelligence is and means as a specific type of behavior in a more inclusive scheme of behavior.

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ON SECULAR COOLING AS AN ILLUSTRATION OF THE METHODS OF APPLIED MATHEMATICS

FRIENDS and critics to whom I have shown my article on "The Methods of Applied Mathematics"[1] have usually made remarks to the effect that the most interesting part of the essay would be its application. It is one thing for the philosopher to abstract and classify the methods of the various specialist sciences, and quite another for him to show that this process is of definite practical use, by giving examples of the manner in which the scientific specialist makes grave and serious errors through ignoring the rightful methods of his science. This essay is an attempt to accomplish the latter purpose.

Amongst the examples of the misapplication of the methods of applied mathematics, none is more interesting or more cogent than that concerning the secular cooling of the earth. Owing to recent discoveries in radioactivity, the discussion has become, in some respects, of purely academic interest. But many of the old fallacies are still extant, and, to act as a check on future errors of the same kind, it will serve a useful purpose to examine the fallacy from the foundation and to show in what manner it arises from the misapplication of mathematical method.

The basis of this well-known method of Lord Kelvin[2] may be briefly described as an attempt, from observations of the increase in temperature as we penetrate into the crust of the earth, to calculate the time that had elapsed since the consistentior status, or, in other words, since a permanent solid exterior first formed on the surface of the earth.

The present essay is not concerned with the calculation, but with the premises on which it is based. It is intended to show, not only that this speculation has been proved invalid by recent discoveries, but that, if the necessary limitation of mathematical methods had been clearly understood, it would never have been put forward.

[1] This JOURNAL, September 30, 1909.

[2] See *Trans. Royal Soc. Edinburgh*, Vol. 23, republished in "Mathematical and Physical Papers," Vol. 3, to which paging references are made.

The basis of the calculation is an equation of Fourier, which was designed as a solution of the hypothetical problem of "the rate of variation of temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two constant values on two sides of an infinite plane."[3]

I do not think that the essential conditions for the earth, in the vast unknown time since the consistentior status, to approximate to this ideal, have ever been clearly and explicitly stated. Some of them will appear at first sight to be truisms and some have been overlooked as a matter of habit, because, in the vast majority of the operations of mathematical physics, they are of the obvious kind that can be tacitly assumed.

The following statement of these implications makes no pretense to be exhaustive; but it will indicate that, in such a case as the present problem, much is liable to be assumed without sufficient and adequate examination. In order that Fourier's equation should be applicable to the concrete case of the real earth during geologic time, this real earth must satisfy, amongst others, the following conditions: (1) The spherical rotating body must for practical purposes correspond with this infinite plane. (2) This same earth must have constant conductivity and specific heat both in time and place. (3) The earth must be mathematically solid. There must be an absence of convective action both in time and place. (4) The temperature of the surface and of the heated interior at a given distance from the surface must be constant for both time and place. (5) The variations in temperature gradient must depend on conductivity and specific heat only, and the process of averaging the varying temperature gradients must be valid. (6) There must be no source of heat other than that originally contained in this solid of known temperature and known specific heat.

It is highly probable that, in any ordinary instance where conductivity formulæ would be used, these assumptions would be valid. In the ideal case of the cooling, within reasonable limits of time, of a large suddenly extruded mass of lava, no reasonable objection could be raised on any of these grounds. But, in the case of the earth cooling through geologic eras, we shall find that no single one of them is entirely satisfied, and that some occasional errors are so great as to invalidate the calculation. I have no competence (and if I had it would be outside the scope of this paper) to examine the necessary modifications dependent upon each of the sources of error; but ordinary application of common-sense reasoning will show

[3] *Ib.*, p. 301.

that, with some of them, a small variation from mathematical exactitude will, if acting for a sufficient time, make great differences in the result. To show this we will examine our six necessary conditions separately.

1. The question of the earth as a sphere is too mathematical for treatment here. This particular difference from ideal conditions does not appear to affect the argument if the others are satisfied; but Professor Perry has shown that, if we assume a higher conductivity for the earth's interior, the spherical nature of the earth must be taken into account.[4] This source of uncertainty, however, is small compared with those that follow.

2. The criterion of constant conductivity is unproven and it is difficult to see how proof can be obtained. On so speculative a question, no definite conclusion seems possible, and the only methods of attacking such a problem would seem to be found in general arguments based on probability. I doubt if it is possible to state all the conditions that might conceivably affect the heat conductivity in the interior of the earth. There are so many unknown factors. But we can reasonably be sure that it will be affected by at least the following three: change of chemical constitution, rise of temperature, enormous increase of pressure. Let us examine the effect of each of these factors.

It is superfluous to remark that we have no direct knowledge of the earth's interior, or that we can only speculate. But, in any speculation of this kind, we are entitled to take as our starting-point the ascertained fact of the high density of the earth (circ. 5.2) which is about double the estimated density of the crust.

This higher density may conceivably be due to one of two causes, either change in chemical constitutions, or a great change in volume due to the superincumbent pressure. Of these possibilities the former would appear to be the more probable. There are good theoretical reasons for thinking that there is a limit to the compressibility of any substance, however great the pressure may be. If this change in density be, in the main, due to change of composition, it follows that the substances in the interior of the earth must be metallic.[5] There are no known non-metallic substances of a density greater than 5. The presence of any considerable proportion of metal would enormously increase the conductivity.

Change of pressure *per se* would undoubtedly increase the con-

[4] *Nature*, Vol. 51, p. 225.

[5] As a speculation, not essential to the argument, it is allowable to surmise that there may be a considerable proportion of iron, for three reasons: (1) on account of its large proportion in the earth's crust compared with other metals, (2) on account of the existence of terrestrial magnetism (this is perhaps the weakest argument), (3) by analogy with meteorites.

ductivity, and, if the greater density of the interior were due to difference of pressure, this increase would be very great. This statement will need small emphasis. In the absence of chemical or intramolecular change, it is obvious that more compact packing of the molecules will facilitate the passage of heat. Plain evidence of this action is shown in the increase of electric conductivities of metals with fall of temperature. With the comparatively small contraction of volume, we find an increase of electric conductivity very much greater than the contraction in volume. The variations in this true electric conductivity[6] are always accompanied by corresponding, though not necessarily proportionate, variations in heat conductivity. Indeed, I do not think any competent chemist or physicist will be inclined to dispute the conclusion that pressure and consequent decrease in volume will probably have the effect described.

The effect of increase in temperature is doubtful. The conductivity of metals decreases with rise of temperature, but the change is small compared with their much greater intrinsic conductivity. The evidence with regard to the ordinary rocks is not conclusive. Lord Kelvin[7] thought that their conductivity would decrease with rise of temperature. Professor Perry[8] is inclined to form the opposite conclusion. In all probability the problem will merge itself into the larger one of the state of matter at abnormal temperatures and pressures. Such a difference between competent specialists is a cogent illustration of the uncertainty of the data on which the method is based.

These three sources of difference, so far as present knowledge will carry us, would affect the conductivity of the interior of the earth in the following way:

Cause	*Effect* (on conductivity)
Change in chemical composition with a greater metallic content.	Great increase.
Increased pressure.	Increase (or with great compression considerable increase).
Increased temperature.	If metallic decrease, but small in proportion to intrinsic conductivity. If non-metallic, doubtful.

The balance of this hypothetical evidence, therefore, clearly inclines to an increase of conductivity of an unknown amount. To hypothetical evidence we can add the positive evidence that, accord-

[6] This argument, needless to say, refers to true electric, not to electrolytic, conductivity.

[7] *Nature*, Vol. 51, p. 439.

[8] *Ib.*, p. 583.

ing to Professor Schuster, the electric conductivity must be considerably greater inside the earth.[9] Thus the conclusion seems highly probable that the heat conductivity of the interior of the earth is considerably greater than that of the crust.

It will throw an interesting light on the present line of reasoning to note the effect of this probable conclusion on Lord Kelvin's estimate of geologic time. Professor Perry[10] has shown that the estimate would thereby be multiplied by a factor "two to six times the ratio of this internal conductivity to the conductivity of the skin." This interesting result is a striking instance of the manner in which, by the use of complicated mathematical formulæ, errors in the data produce a more than proportionate error in the result.

3. The problem of the solidity or fluidity of the earth's interior is generally supposed to have been solved by observations on the behavior of the earth in its reactions to tidal forces. On this subject Lord Kelvin[11] expresses the conclusion that "On the whole we may fairly conclude that, whilst there is some evidence of a total yielding of the earth's mass, that yielding is certainly small, and the effective rigidity is at least as great as that of steel." Elsewhere he says that this rigidity is certainly more than "a continuous globe of glass of the same diameter and probably than one of steel."[12] Sir George Darwin[13] is of the same opinion and thinks it probable that "the earth is solid at least far down to its center." Against this conclusion can be brought the simple observed fact of the extrusion of lava.

In the face of these observations, it will at first sight appear absurd to maintain the possibility of convective action in a body probably as rigid as steel. But, for the purposes of geologic time, these facts prove very little. It is only necessary to inquire exactly what we mean by liquid convection and how much of this will render a conductivity formula useless, to see that these observations are by no means as conclusive as they seem.

When we have proved that the earth is effectively rigid in its reaction to tidal forces and in its behavior with regard to the transmission of earthquake shocks, it may yet be effectively fluid or viscous with regard to long-continued forces acting for vast eras of time. It must be borne in mind that, in speculating on the condition of matter in the interior of the earth, we are treating of conditions which lie beyond the range of present-day science. Of these condi-

[9] *Phil. Trans.*, Series A, 1889, p. 512.

[10] *Ib.*, p. 225.

[11] "Mathematical and Physical Papers," Vol. 3, p. 329.

[12] *Ib.*, p. 237.

[13] "The Tides," pp. 232–237.

tions we know little, but we can infer with a fair degree of certainty the existence of a very high temperature and of a colossal gravitational pressure. It is evident that it is an inadmissible extrapolation to dogmatize with regard to the possible reactions of matter under conditions so far removed from those of which we have experience.

Interesting and valuable as these researches on the tides may be, they in no way negative the idea that there may be slow convective action throughout the whole interior mass of the earth. Let us imagine such an action as slow as a foot a year. This, in the course of 20 million years, would carry a mass of material from the center of the earth to the circumference. Indeed, convection to the extent of a foot per century would probably entirely vitiate such a calculation as the one we are now considering. Any such action, even as slow as this, would connote a continual circulation of the mass, and, for the purposes of geologic time, would be equivalent to a great increase of conductivity at depths where the heat begins to approach that of the interior.

To any suggestion that this argument is hypothetical, I would at once admit that, were I attempting to found complex mathematical calculations on these suppositions, such speculations would be inadmissible; but the suggestions are no more speculative than the inference that, because the earth is rigid in its reactions to tidal forces, therefore no such reactions exist. The indefinite grading of the properties of solids and liquids here suggested is indicated by more than one branch of chemical fact and theory.

The fallacy of ignoring the possibility of liquid convection in the interior of the earth is a cogent example of the manner in which factors, commonly negligible, become important when we attack a problem of an entirely different order. Because, in studying the cooling of a cannon-ball, we can ignore any minute liquid convection that may exist, it is assumed that this may still be ignored when we study the cooling earth. But the conditions are entirely different. The small intrinsic conductivity of rock and the enormous mass of the earth making cooling so slow, has reduced the conductivity effect to an infinitesimal. But, when that is done, we must also take account of other factors which, because they also are infinitesimals, may commonly be ignored. In studying the cooling cannon-ball we can also ignore the effect of gravitation; but, in the cooling sun, gravitational friction will supply heat for many millions of years.

The former paragraphs refer only to variation of conductivity and convective action in space. There still remains the question of such variation in time. Even if we admit the improbability of such convective actions now (and there is no reason for such an admission) that assumption by no means precludes their possibility in the

remote past. If this is so, our present temperature gradient would have reference, not to the consistentior status, but to the time when such convective actions ceased, which, for all we know, might have been many millions of years after the original cooling of the earth's crust.

The specific heat of the earth is another problematic factor, but one which is probably of trivial importance.

5. The sources of errors in connection with the determination of temperature gradients are considerable and raise some theoretical questions concerning the conditions under which the taking of an average is permissible. Without attempting to enter very thoroughly into these, it will be generally admitted that this process is inadmissible when the variations are large and when their causes are known. In Lord Kelvin's original papers and, so far as I have been able to ascertain, in any subsequent papers, there is no serious attempt to deal with this problem. There is in one passage a suggestion that some particular variations may be due to the residual heat of an eruption,[14] but, as we shall see later, this, if true, would invalidate the calculation of an average. We now know that at any rate a part of these variations is due to differences in the radioactive content of the rocks, but this question I propose to defer for the present.

Apart from the newly discovered effect of radioactivity, there are at least two known possible reasons for the enormous variations in gradients (from $\frac{1}{15}°$ F. per foot to $\frac{1}{100}°$ F. per foot): differences in conductivities of rocks and other means by which heat may escape, and the residual heat due to the eruptions of former times. It is evident that, while it would be allowable to take an average of variations due to the first cause, it would be entirely inadmissible with regard to the second, as all such variations of temperature gradient would be positive. So far as the variations were due to that cause, the correct data for a mathematical calculation would be not the average, but the lowest gradient that could be found.[15]

The magnitude of variations due to volcanic action past and present has never been estimated, nor—now the problem is complicated by radioactivity—does there appear to be much chance of our so doing; but the uncertainty suggests a number of speculative questions to which some answer must be obtained before it would be possible to admit the validity of the usual deductions from temperature gradients. That the necessary data are unproved may be

[14] "Mathematical and Physical Papers," Vol. 2, pp. 176–177.

[15] Recent borings, made with due regard to some possible sources of error show a tem. grad. of 1 in 77. This correction alone would more than double Lord Kelvin's estimate (Chamberlain and Salisbury, "Geology," Vol. I., p. 543. N. B. $(77/55)^2 = 2.4$ nearly).

shown by the following queries, which raise a few of the unsolved geologic problems that have some bearing on this question: Is there a single locality on the surface of the earth where, if we go deep enough, we should find no traces of igneous rocks formed since the consistentior status?[16] What is the range in place and time, of the effect on the temperature gradient of the existence of a group of volcanoes? (Observations from southern Italy might throw light on this problem.) In what ways and to what extent may temperature gradients be modified by igneous action that does not reach the surface? In what way, if at all, will continuous erosion and sedimentation modify any conclusions that may be drawn? The effects of this extend many miles deep in the earth's crust—as a rule, deeper than our observations of temperature gradients. As the possibility of slow interior convection vitiates the assumption of a mathematically solid earth, so the fact of such convective action in the crust may vitiate the assumption that the gradients that have been observed may be averaged.

Indeed, in this case, immediately we apply a single mathematical formula to such a vast expanse of time, we see that some of our original assumptions must be reconsidered. Let us imagine ourselves on a time machine which would cover a hundred thousand years in what to our senses was a day, and we shall see that our fixed and steady earth, cooling as if it were a rigid globe of glass or steel, is a figment of the imagination. In the place of this still fixed solid earth we should see a globe, the center of plutonic energies. Its surface would rock beneath our feet like the billows of the sea, literally mountains high, and as it rocked, the solid land would pour into the ocean in one mighty flood. There would be vast eruptions from innumerable centers forming lava covering great areas which would once more be plunged beneath the surface of the ocean. Is it possible for all these changes to take place and yet leave the heated interior mass so unaffected that we can calculate how long it has taken to cool on the assumption that it was fixed and steady? Where is the original rock that first cooled in the remote past? Where are we to look for our data if we wish to make our calculation?

To sum up our analysis of this particular method, so far as possible, apart from recent discoveries in radioactivity, it will be seen that the premises are subject to a large number of uncertainties. Assumptions have been made on matters about which we have very imperfect knowledge, and our present inquiry has disclosed the existence of a large number of variable and doubtful factors which

[16] Recent investigations have shown that the great granitoid series, found in the Archean complex, are not the original crust of the earth, but are mainly intrusive in the surrounding metamorphic schists.

make this method entirely inadmissible for the calculation of a maximum limit to geologic time. Assuming the non-existence of any other source of heat, the utmost maximum that could be obtained from the known facts would be referred not to the temperature gradient, but the plain fact that volcanic action is still going on, and that consequently a considerable fraction of the original heat is still existent in the earth's interior. On this basis, if we accept Lord Kelvin's estimate[17] that such original heat is equal to twenty thousand million times the amount dissipated each year, our only calculable maximum would be something less than this enormous lapse of time. We must subtract something from this maximum to allow for more rapid radiation in past eras, and for the fact of observation that the interior of the earth is still hot. How much should be deducted on these counts is an academic question that I shall not attempt to solve. Whether this is a probable or even a possible maximum is also a question that does not arise at this point. But, apart altogether from the discovery of radioactivity, the temperature argument appears to be so faulty that this is the only allowable maximum result deducible from the study of the secular cooling of the earth.

With the introduction of more recently discovered factors, even this residual validity vanishes. The two known sources of internal heat discovered since the publication of Lord Kelvin's papers are internal tidal friction and radioactivity. Sir George Darwin has shown that the heat equivalent of the loss of rotational energy of the earth, owing to tidal friction, if the earth had originally a day of 5 hours 40 minutes, would be equivalent to a secular loss for 3,560 millions of years.[18] He gives a number of mathematical calculations to show that the greater part of this heat would be formed in the central parts of the earth,[19] and so he comes to the conclusion that this would not appreciably affect the temperature gradient near the surface.[20] But this conclusion, like so much other mathematical reasoning, implies an entire absence of interior convective action both now and in the remote geologic past. If we reject this implication, this tidal heat thereby becomes a factor of which we must take account in our study of secular cooling.

Careful investigation of the radioactive content of the igneous and sedimentary rocks has shown that, if similar action occurred in the interior, the heat evolution from this source alone would be many times greater than the amount lost by secular cooling. Into this matter we need not now enter. The discovery of radioactive heating

[17] "Popular Lectures and Addresses," Vol. 2, p. 6.

[18] *Philosophical Transactions*, Series A, 1879, p. 592.

[19] *Ib.*, p. 593.

[20] *Ib.*, also pp. 564–566.

is one that no scientist of the past century could be expected to have anticipated. It is, however, a very cogent example of the shallowness of this kind of mathematical speculation that it was so ready to assume that there existed in the universe no sources of energy other than those with which we were acquainted.

Let us now show the theoretical reference of this more practical analysis. In my paper on the "Methods of Applied Mathematics" I summed up the most probable sources of error in the fitting of conceptual mathematics to perceptual science in the following four propositions: (1) the validity of mathematico-physical formulæ is dependent on the range and theoretical certainty of the physical or other principles involved in the premises; (2) it is also dependent on the completeness with which the conditions relevant to any particular problem have been abstracted from reality; (3) both these sources of error are increased with the complexity of the formulæ and with the magnitude of the substituted terms; (4) in cases of extensive extrapolation, such formulæ are liable to give not only minor errors, but results which bear no recognizable relation to concrete reality.

Applying these tests to secular cooling, we find: (1) that, although the full discussion of the theoretical foundation of Fourier's formula is too detailed a mathematical question for the present essay, it contains physical assumptions not of the order of axioms; (2) in its application to geologic time, the abstraction is entirely inadequate, some known, and probably many unknown, material factors have been omitted; (3) the formula is complex, small errors in the data make considerable errors in the result, and the extrapolation enormously exceeds the ordinary limits of space and time; (4) it is therefore highly probable, from these theoretical considerations, that the conclusions bear no recognizable relation to concrete reality.

Let us note the special interest of these conclusions. On the one hand, we have an exposition of the principles of applied mathematics which contains truths so clear that the philosophical critic is inclined to dismiss them with the remark that they are true but obvious. On the other hand, we have a theory which, during the past half century, has filled countless pages in books and scientific articles, molded the course of scientific and geological speculation, effected theories of evolution and of heredity, and entailed enormous labor on the part of mathematicians, scientists and writers (to say nothing of students, readers and mechanical workers). To all this, given the clear understanding of a few simple mathematical principles, it is possible to make the single exhaustive comment: one erasure will suffice. What more cogent example could be desired of

the need of a thorough investigation of the theoretical principles of scientific method? We require less mechanical experiment, less mechanical mathematical calculation, more thought, more philosophy.

With the detailed working out of one example, it is hardly needful to give further examples of the misapplication of mathematical method. The tidal-retardation fallacy is another example more obvious than the last, so clear indeed that to apply these principles in detail is almost pedantic. This example, however, is of special interest in that recent discoveries in radioactivity do not affect the argument. This case it is unnecessary to discuss further, because the mathematicians have already discovered their errors by empirical methods. To elaborate other instances would be superfluous. I can, indeed, best conclude this esay, as I concluded my exposition of main principles, by saying that all acquainted with the multitudinous applications of mathematical reasoning to the complexity of the universe around us can, for themselves, find many other examples.

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DISCUSSION

IF THE BLIND LEAD THE BLIND

A COMMENT ON "LOGICAL FORM" IN PROFESSOR PERRY'S "REALISTIC PROGRAM"[1]

WHEN will philosophy tire of that Fata Morgana, mathematical method? It is little short of tragic if realism, bearing, as it does, balm for so many ills to which the philosophic flesh is heir, theory of relations, the theory of 'logical constants' or indefinables, the theory of infinity and continuity, and the theory of classes and principal contribution which modern logic is prepared to make to philosophy concerns the form of exact knowledge," and also: "The should be led astray. But I am apprehensive when told that "The systems, concern everything fundamental in philosophy. No philosopher can ignore these and like theories without playing the part of an amateur. . . . For philosophy deals with the same topics as modern logic, but treats *popularly and confusedly* what modern logic treats with the *painstaking thoroughness and exactness* of the expert"[2] [italics mine].

Before these statements become incorporated in a realistic platform, I should like to present the three following theses, with the proviso that it is not as a subordinate philosophic discipline, as a

[1] This JOURNAL, Vol. VII., No. 14, July 7, 1910.

[2] *Loc. cit.*, p. 374.

branch of logic, that the investigation in question is invalidated, but only as a discipline peculiarly suited to contribute to the solution of the philosophic problem:

1. The unclearness of mathematical logic and the lack of agreement in its usages make all efforts to transfer its concepts to philosophy impracticable.

2. Even if these concepts were clearly defined, their specific relation to the logic of mathematics would make the transfer of them a neglect of "the division of the question."[3]

3. Even if these concepts were clearly defined and transferable, the type of method set by mathematics is suitable only for the critical recasting of philosophic systems and not for a developing philosophy such as modern realism.

I

To one who has followed the literature of the subject there is no need of recalling the unclearnesses of the concept in question. M. Lucas de Pesloüan has exhibited them with considerable detail in a series of articles reviewed in this JOURNAL,[4] and prior to 1907 they have furnished MM. Poincaré and Couturat an ever-fresh bone of contention in the pages of the *Revue de Métaphysique et de Morale* —until the latter became too deeply absorbed in the problem of international language to carry on the debate. However, there may be those to whom a few examples will not be amiss:

Logical constants—"Logical constants are all notions definable in terms of the following: . . ." (Russell, "Principles of Mathematics," p. 3).

"Thus pure mathematics must contain no indefinables except logical constants" (*idem*, p. 8).

"The particular notions which appear in the propositions of symbolic logic, and all others definable in terms of these notions, are the logical constants. The number of indefinable logical constants is not great" (*idem*, p. 11).

Logical constants are then definable, indefinable, some definable and some indefinable. And why not both and neither? Incidentally, "A constant is to be something absolutely definite, concerning which there is no ambiguity whatever" (p. 6). It is interesting to collate different passages concerning the specific logical constants.

Infinity—This term has unambiguous definitions in two distinct

[3] "Exact knowledge must be precisely limited in its application. A disposition in philosophy to employ terms in an unlimited sense, and to make unlimited assertions, is doubtless the principle reason why philosophy at the present time possesses no common body of theory."—Perry, *loc. cit.*, p. 375.

[4] Vol. V., pp. 21–25; Vol. VI., pp. 610–612.

usages, as ordinal and as cardinal infinity. But starting from these definitions, mathematicians have attained a wealth of paradoxes concerning the transfinite before which Bolzano's *Paradoxien*[5] pale like Halley's comet before the sunrise. M. Poincaré[6] has claimed that they are rooted in the assumption that an infinite collection exists, and I also have tried to make manifest the ambiguity involved in the so-called "existence proof."[7]

Class—Here is a central difficulty. Peano[8] merely calls a class any collection of objects. Russell[9] says a class is "all the terms satisfying some propositional function." But Jourdain,[10] because of the discovery (!) of nonentities that make null classes look like real classes, puts the question whether every propositional function really does determine a class, and Russell himself[11] in a frantic attempt to get rid of certain paradoxes, proposes (1) a zigzag theory of classes, *i. e.,* some propositional functions define classes and some do not, although apparently you can't tell which is which until you try—and then you are likely to be wrong, (2) a theory of the Limitation of Size, *i. e.,* a class is not a class if it gets too big, and (3) a No-class Theory, *i. e.,* there aren't any such things, anyway. Now,[12] he is proposing a theory of logic based on a theory of types, but Poincaré says[13] that this attempt is very unclear in its distinctions of the finite and transfinite.

This section could be continued *ad infinitum.*

II

The limitation which their specific purpose imposes on these concepts makes them unsuited to philosophy.

Consider such a simple notion as that of equality. Whitehead[14] says: "When two groups of symbols are connected by this sign (=), it is to be understood that one group may be substituted for the other whenever either occurs in the calculus *under conditions for which the assertion of equivalence holds good*" (italics mine), *i. e.,* when you can. This is not on every occasion. For example, let

$$f(x, y) = 0,$$

[5] B. Bolzano, "Paradoxien des Unendlichen," Berlin, 1889.
[6] *Rev. d. Mét. et d. Mor.,* 1906.
[7] This JOURNAL, Vol. 5, 1908, pp. 628–634.
[8] Notations, "Form. d. Math.," t. I., p. 4, 1904.
[9] *Op. cit.,* p. 19.
[10] "De Infinito in Matematica," *Red. d. Mat.,* t. VIII., 1906.
[11] Reviewed here, 1906, pp. 388–390.
[12] *Am. Journ. of Math.,* Vol. XXX., 1908.
[13] *Rev. d. Mét. et d. Mor.,* 1909, pp. 451–482.
[14] "Universal Algebra," p. 5.

then

$$\frac{dy}{dx} = -\frac{\frac{\delta f(x,y}{\delta x}}{\frac{\delta f(x,y}{\delta y}},$$

but it is not possible to write $-\frac{\delta o/\delta x}{\delta o/\delta y}$. And this notion is too narrow to be transferred to other mathematical systems.

The mathematical linear continuum is a defined collection of entities such that if they are correlated with possible cuts in a straight line, there will be one-one correspondance, *i. e.*, there will be no entity to which one and only one possible cut does not correspond, and no cut to which one and only one entity does not correspond. For some purposes, then, the mathematical linear continuum is a substitute for the line, and we say the line is, in so far forth, a linear continuum. But no linear continuum, merely taken as such, ever has direction. Direction is a rather significant characteristic of lines. It may be doubted if the mathematical notion of the continuum has the value in philosophy of Aristotle's simpler notion of parts having a common boundary within a whole. There has been one great attempt to take over the mathematical concept of infinity into philosophy—Professor Royce's absolute idealism. Readers of that philosophy can judge of its success. It is certainly an advantage to free oneself of the notion that the infinite is *per se* contradictory, if one ever had that notion, but the gain in drawing a parallel between "the structure of the number series and the bare skeleton of the ideal self"[15] is not so evident. Philosophers have not been brought to perfect agreement even as to the advisability of asserting that "the Absolute is a Kette."[16] There is certainly a suspicion possible that this mathematical concept, defined in a specific sense, is not significant, even if correctly taken over into philosophy.

III

Mathematical method, as such, is suited only for a critique of a developed system.

Dr. Karl Schmidt in an interesting paper in this JOURNAL[17] has presented the thesis that the most important thing for present-day philosophy to do is to "make up its mind" concerning certain philosophic issues, *i. e.*, get definite "generating problems" and develop them into a philosophy. When developed the same able thinker has

[15] "World and the Individual," Vol. I., p. 527.

[16] *Idem*, p. 546.

[17] 1909, pp. 673–685.

proposed a Critique of Cognition that may be applied to them.[18] This critique has been developed from the study of scientific method but not taken over from it. With this procedure the present writer is in entire sympathy. Therefore, if there is to be a realistic platform, the agreement should be primarily one of fundamental opinions. The method of the development is insignificant compared with the attainment of results, and as Dr. Schmidt has so clearly seen, the logico-mathematical form is suited to criticism only, not to development.

Suppose it were made the method of development. Let us assume that realists are agreed on a complete set of postulates. Their task is then to deduce the consequences of these postulates, manipulating them according to the laws of the system of formal logic agreed upon. The result would be worse than the worst type of scholasticism. Either ethical, political, sociological, and religious views are independent of philosophy—and in that case what significance can philosophy claim?—or else the realists would all say the same things about these issues. What a dull place the world would be if every one were converted to the cause! Each man's life would be playing his part in realizing the deduced consequences of realistic postulates. Common points of view, tendencies, are healthy; but mathematical agreement of opinions is disease.

And the sole method of philosophy would be dialectic, although dialectic has never convinced anybody. It has silenced many. Professor Royce founds his system on irrefutable dialectic—on propositions the very denial of which reaffirms them. But Professor Spaulding[19] refutes him. It would be an interesting exercise in dialectic to refute Professor Spaulding in turn. It is probably possible, as no dialectic argument in the history of philosophy has ever won uniform assent from those to whom it was addressed, and they have not always been dull.

The above comment suggests the sad conclusion that philosophers must do the work of philosophy. No other science is going to do it for them. However complex or however simple concepts may be, their form must be studied in relation to the facts they are intended to interpret. The great merit of Kant is that he made up his mind as to his "generating problem" and adapted his method to its solution. The solution has difficulties, but it is great. We reject it, not because it is bad philosophy, but because his "generating problems" are not ours. If our civilization resembled more closely that of Kant's country and day it would be a different story.

[18] *Idem*, pp. 281–287.

[19] *Phil. Rev.*, Vol. XIX., No. 3, 1910, pp. 276–301.

And perhaps the unclearness of philosophy that so troubles the realists is not due to the fact that we have not absorbed mathematical logic, or any other science, but arises because we have allowed sciences, cuckoo like, to lay their eggs in our nest and have unresistingly brooded upon them until the hatchings startled us from our lethargy. By all means we should aim at clearness, precision, and all the other virtues—we have, all of us, too few of them. And we need to know as much about the achievements of science as is possible. But the conceptual synthesizing which must interpret this knowledge and make it into philosophy—for no mere heap of knowledge is philosophy—is the work of the philosopher and his concepts must be adapted to its specific needs. It is time to seek philosophic problems and philosophic methods for the philosopher.

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REVIEWS AND ABSTRACTS OF LITERATURE

Esthetic as Science of Expression and General Linguistic. BENEDETTO CROCE. Translated by DOUGLAS AINSLIE. London: Macmillan & Company. 1909. Pp. xxxi + 403.

Mr. Ainslie has translated the whole of the theoretical part of Croce's "Estetica" and has presented in English an abbreviation of the historical part. An appendix contains a translation of an address on "Pure Intuition and the Lyrical Character of Art," delivered by Signor Croce at Heidelberg at the Second General Session of the Third International Congress of Philosophy. This lecture includes a brief presentation of Croce's theory and might well be read before the larger work. The translator's introduction to the "Æsthetic" is a glorification of Croce and all his works. He is hailed as "one of the very few great teachers of humanity." His "Logic" and his "Philosophy of the Practical," which, with the "Esthetic," make up his "Philosophy of the Spirit," are praised in the highest terms. He is the "Columbus" of esthetics. Signor Croce is known to English readers as a critic of the first rank and as the author of a work on esthetics which has attracted much attention, but few were prepared to hear him praised in such extravagant language.

For the most part the translation is very close to the original, in fact it is almost literal. There are, however, occasional departures which modify somewhat the meaning of the author, as, for example (p. 18), where the expression "indifferente alle discriminazione posteriori ed empiriche della realtà ed irrealtà" by "indifferent to discriminations, posterior and empirical, to reality and to unreality."

On the other hand, faithfulness to the original can hardly justify a sentence which begins, "It is from human psychology, that is, literary psychology, that comes the other objection, to the effect," etc. And it is

difficult to see what is to be gained by using constantly such an expression as "*posing*" a problem, or such a term as "*equivoque.*" The reader is very frequently put to unnecessary labor in his attempts to reach the author's thought. The style of the original is also open to criticism. The terminology is somewhat Germanized. The translator exonerates the author from the charge of Hegelianism on the ground that he has gone beyond Hegel. Croce's esthetics is not Hegelian, but his thinking contains much that comes from Hegel and Kant. It is occasionally necessary to overcome some slight irritation at the author's uncompromisingly authoritative tone in the statement of his own views and in the criticism of the views of others. These various obstacles should not, however, deter the reader from the examination of a book which has aroused more interest than any other recent work on esthetics.

Since the time of Kant the investigation of esthetic problems has usually led more or less directly to the discussion of certain mental states. The beautiful and the sublime have been defined with reference to the feelings which they arouse. The beautiful, for example, has been defined as pleasure objectified, etc. Signore Croce carries subjectivism in esthetics to a point far beyond any which it has previously attained. Others have resolved beauty into something wholly within the mind; Croce goes farther and makes *the whole of art* entirely an affair of the inner world. Art is identified with the "esthetic vision"; it is "entirely internal." What we ordinarily call art is for him merely the *externalization* of art.

The work in which this doctrine is presented is very broadly conceived. It touches many of the departments of human activity and contains, indeed, a summary sketch "of the entire philosophy of the spirit in its fundamental moments." All human activity is either *practical* or *theoretical,* the practical having to do with the will, the theoretical having to do with knowledge. Practical activity is either *economic* or *moral.* Knowledge is either *intuitive* or *logical.* "The spirit is conceived as consisting of four moments or grades, disposed in such a way that the theoretical activity is to the practical as is the first theoretical grade to the second theoretical, and the first practical grade to the second practical. The four moments imply one another regressively by their concretion. The concept can not be without expression" (*intuition*), "the useful without the one and the other, and morality without the three preceding grades." Intuition is thus made fundamental; it is independent of logical knowledge. Concepts may sometimes be present in intuition, but they are present not as concepts, but as simple elements of intuition. Moral maxims in the mouth of a character in a tragedy are present not as concepts, but as characteristics of the speaker. An intuition is a complete synthesis. Concepts and associations *as such* are not present in intuition. When they are present at all they are welded into a single whole of intuition. Pure intuition has no reference to reality or non-reality. "Intuition is the undifferentiated unity of the perception of the real and of the simple image of the possible." "All intuition is expression," expression in some kind of material, verbal, pictorial, musical, or what not. The proof which the author gives of this identification (pp. 13–14) is neither

convincing nor necessary. As a matter of fact, his statement is a definition and not a conclusion. Expression means not expression to another, not external manifestation, but internal expression, presentation to one's own mind, simply intuition.

As we have already seen, art is identified with the "esthetic vision," and this proves to be nothing other than expression or intuition. All intuition is then art or esthetic intuition. What we ordinarily include in the latter differs from the rest only in quantity or extensity; it is exactly the same in kind. The various steps from bare impression to the external manifestations which are ordinarily called works of art is stated as follows:

"The complete process of esthetic production can be symbolized in four steps, which are: (*a*) impressions; (*b*) expression or spiritual esthetic synthesis; (*c*) hedonistic accompaniment, or pleasure of the beautiful (esthetic pleasure); (*d*) translation of the esthetic fact into physical phenomena (sounds, tones, movements, combinations of lines and colors, etc.). Any one can see that the capital point, the only one that is properly speaking esthetic and truly real, is in that (*b*) which is lacking to the mere manifestation or naturalistic construction, metaphorically also called expression."

The esthetic fact is form and nothing but form. It is form as distinguished from content, from matter, when "matter is understood as emotivity not esthetically elaborated, that is to say, impressions, and form, elaboration, intellectual activity and expression."

"Art is the expression of impressions" not of ideas (nor even of emotions, for emotions themselves would have form, and would, we conclude, be themselves art). Impression is apparently to be taken in the Kantian sense. Unity is an essential characteristic of the work of art.

"Another corollary of the conception of expression as activity is the *indivisibility* of the work of art. Every expression is a unique expression. Activity is a fusion of the impressions in an organic whole. A desire to express this has always prompted the affirmation that the work of art should have *unity,* or, what amounts to the same thing, *unity and variety.* Expression is a synthesis of the various, multiple, in the one."

From the identification of art with "spiritual esthetic synthesis" and from the further statement that there are no subordinate kinds of esthetic syntheses, it follows that the ordinary divisions and classifications of art have nothing to do with esthetics proper. They have to do only with the externalization of art.

Beauty is defined as "*successful expression,* or better, as *expression* and nothing more, because expression, when it is not successful, is not expression. Consequently, the ugly is unsuccessful expression. . . . In works of art that are failures, the beautiful is present as unity and the ugly as multiplicity."

Such concepts as the sublime, comic, ludicrous, and so on are regarded as pseudo-esthetic. "The facts, classified as well as possible in the above-quoted psychological concepts, bear no relation to the artistic fact, beyond the generic one, that all of them, in so far as they designate the material

of life, can be represented by art; and the other accidental relation, that esthetic facts also may sometimes enter into the processes described, as in the impression of the sublime that the work of a Titanic artist such as Dante or Shakespeare may produce. . . ."

What is ordinarily called beautiful or physically beautiful is simply the stimulant of esthetic reproduction. This statement applies to what we usually call works of art as well as to natural objects.

These in brief are the main elements in Croce's own doctrine. His work is valuable, however, not simply for the theory which it presents, but for the many suggestive views which the author puts forward on most of the subjects related to esthetics. His criticisms of other esthetic doctrines are very outspoken and usually adverse, nevertheless they are stimulating and valuable. In the historical part of his treatise, Croce presents in brief a review of all the historical contributions to esthetics. According to his own statement a historian always adds something to the facts; he is never absolutely impartial. What Croce himself adds (or omits) is determined partly by his own doctrine and partly by his admiration for Vico and a few other Italian contributors.

The main question with regard to Croce's esthetic doctrine seems to the reviewer to be this: Is the identification of art with a purely subjective state the most effective means for organizing the facts which fall within the field which is generally recognized as belonging to esthetics? Can the question of the meaning and significance of what Croce calls the "externalizations" of art, can the problems connected with the production of art and the nature of the artistic genius, can the questions occasioned by every consideration of the esthetic experience, be handled any more successfully on this theory than on any other that has been presented? To the present writer the great value of Croce's work seems to lie in its freshness and in its many stimulating suggestions, not in the novelty which it has nor in the conclusiveness which it has not.

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Old Criticism and New Pragmatism. J. M. O'SULLIVAN. Dublin and Waterford: M. H. Gill & Son, Ltd.; London, New York, Bombay, and Calcutta: Longmans, Green, and Co. 1909.

One closes this book with the sense of having enjoyed with the writer a period of fruitful reflection leading to a riper realization of the meaning of familiar things, yet with a feeling of regret that a work of such fine quality of thought and scholarship should be on the whole so fragmentary in result. The work consists of four essays, two on "Old Criticism," which form its main body, and two on "New Pragmatism," which are brief in compass and rather supplementary in character. So that, while the "and" of the title stands for a certain degree of unity, the addition which it marks renders the unity rather unimportant.

The first essay is the author's doctoral thesis entitled, "A Comparison of the Methods of Kant and Hegel, Illustrated by their Treatment of the Category of Quantity." This occupies two thirds of the book and is a

valuable contribution. Its point of departure, reached after a careful comparison of the standpoints, first of Kant and Hume, then of Kant and Hegel, is Kant's treatment of the categories. This most obscure portion of the "Critique of Pure Reason" is subjected to a patient and searching, yet not unsympathetic, criticism, with reference mainly to the category of quantity, for the purpose of laying bare the actual disconnectedness of Kant's treatment of the categories, along with his sense of their relationship, as shown by many suggestions which were afterwards developed by Hegel. According to the author, disconnectedness between the elements is everywhere characteristic of the system of Kant. Pure reason is itself a rather forced abstraction. Then the logic and the esthetic are left in mutual isolation. When we come to the categories we find that all are supposed to arise from the synthetic unity of apperception; yet in point of fact they are derived in a rather haphazard way from the current forms of judgment. Hence, "the 'metaphysical deduction' was foredoomed to failure, since it reverses the natural order of things." It is in the "transcendental deduction" that we begin to appreciate their systematic relations. From this point he undertakes an exhaustive analysis of the category of quantity, to show that even in Kant there is not only a certain order among the categories, but a certain development, *e. g.*, from unity, through plurality to totality, corresponding, respectively, to Hegel's (1) attraction, continuity or unity, (2) repulsion, discontinuity or amount, (3) limit or number. All of these are interrelated forms of the synthesis of the homogeneous in the given of sense; and thus forms alike of intuition and of thought.

What is to explain Kant's failure to develop the systematic character of the categories? The author gives two reasons: First, because Kant was interested, from beginning to end, in the application of the categories to experience—with the problem of the possibility of synthetic judgments *a priori;* while Hegel was interested in the logical development of the ideas for themselves. Secondly, because of Kant's profound respect for the laws of physical science. For him the forms of science were, after Newton, once for all fixed, while for Hegel they were steps in a still uncompleted critical process.

The second essay is on Kant's Treatment of Causation. Kant's purpose here is "to provide a firm basis for natural science, and at the same time to show the impossibility of freedom as an empirical factor and the futility of the attempt to prove the existence of God by means of the cosmological argument." But Kant adopts a "streak" view of causation, *i. e.*, the cause of an isolated event *b* is sought in another isolated event *a*. This, however, is a very rough-and-ready way of treating the matter. Really, the totality of phenomena at any moment is the cause of the totality of the next moment. And this is presupposed in Kant's proof of the causal axiom. But statements about the world as a totality in fact assume that the world is a thing in itself. Kant is then in an awkward dilemma: he must reject either the principle of causation or else the fundamental idea of his dialectic as a whole. Or, he must either renounce his phenomenalism or else his hope of proving the validity of necessary

synthetic *a priori* judgments. Or again, he must either adopt a streak view of causation or presuppose an "ideally perfect experience" as given. With his failure to prove the absolutely necessary character of the laws of nature, his attack on freedom must also be rejected. And in any case his proof of the causal axiom leaves the inner experience untouched.

The titles of the last two essays are "Pragmatism as an Epistemological Method in its Relation to Criticism" and "General Comment on Criticism and Pragmatism." They are well written, and thoughtful as far as they go, but rather lacking in substance. The author's suggestion that criticism is responsible for pragmatism is not made nearly so strong as it might be. He makes one point, however, which is well worth noting, namely, that a pragmatic epistemology may be adopted in conjunction with almost any brand of metaphysics.

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JOURNALS AND NEW BOOKS

REVUE DE PHILOSOPHIE. March, 1910. *Amour spirituel et synthèse aperceptive* (pp. 225–240): P. ROUSSELOT. – *Amour* (desire or appetite in general) plays a double rôle in the intellectual processes: (1) in the form of the desire for God and for the complete self it is the motive power, the dynamic, active element, of the intelligence; (2) in the form of the desire for the God who is truth, a desire which causes us to see our good in every truth, it effects in us the synthesis of apperception. *La défense de la vie* (pp. 241–261): DR. GRASSET. – A description of the physiological processes of defence against the attacks of extraneous matter and a plea for a similar concerted defence against the enemies of the social life. *L'Absolu. Étude historique* (pp. 262–281): C. HUIT. – An inquiry into the place of the concept of the absolute in the philosophies of Israel, India, Egypt and pre-Socratic Greece. *Chronique pédagogique* (pp. 282–293): G. JEANJEAN. – Short critical reviews of Cramanssel's *Le premier éveil intellectuel de l'enfant;* Dugas's *Le problème de l'Éducation;* A. Binet's *Les Idées modernes sur les enfants;* Foerster's *L'école et le caractere:* Ferrière's *Projet d'école nouvelle;* Gerini's *Gli scrittori Pedagogici Italiani del secolo decimonous;* Vowinckel's *Pädagogische Deutungen;* and of the latest volumes of *Archives de psychologie, Zeitschrift für Experimentelle Pädagogik, Zeitschrift für Kinderforschung,* and *Eos. Etude sur les théories de la connaissance. III. Le Kantisme* (pp. 294–305): P. CHARLES. – The Critique of Pure Reason, summarized in the first portion of the article, may be criticized on the grounds, (1) that the concept of space is acquired through the exercise of the senses—hence is *à posteriori;* (2) that the categories may be explained without resort to innate forms *à priori;* (3) that these subjective forms applied to the world of experience lead inevitably to scepticism. *Analyses et comptes rendus.* L. Prat, *Contes pour les métaphysiciens:* G. BRUNEL. A. Bros, *La survivance de l'àme chez les peuples non-civilisés.* A. Leclère, *Pragmatisme,*

modernisme, protestantisme: F. CHOVET. F. le Dantec, *La crise du transformisme:* J. MARATAIN. G. Kertz, *Die Religionsphilosophie Joh. Heinr. Tieftrunk.* K. Krienelke, *J. H. Lomberts philosophie der Mathematik:* H. OLLION. L. Garriguet, *La valeur sociale de l'Évangile.* K. Oesterreich, *Kant und die metaphysik:* H. LÉARD. *Notes bibliographiques. Recension des Revues et Chronique.*

REVUE DE PHILOSOPHIE. April, 1910. *La vertu et le juste milieu* (pp. 337–346): P. D'HÉRONVILLE. – The Aristotelian conception of virtue as a mean is essentially derived from Plato's theory of the harmonious life, and is the forerunner of the scholastic and christian doctrines. It describes virtue in a common-sense way as habitual action in accordance with reason. *L'Absolu. Étude historique (2e Article)* (pp. 347–376): C. HUIT. – An inquiry into the concept of the absolute in the Socratic and subsequent Greek philosophies, in the scholastic systems, and in continental philosophies up to and including that of Leibniz. *Revue critique de cosmologie. Théories et hypothèses scientifiques* (pp. 377–392): J. M. DARIO. – Recent studies in cosmology, such as A. Rey's *La théorie de la physique chez les physiciens contemporains,* P. Duhem's *La théorie physique, son objet, sa structure,* and Poincaré's *La Science et l'hypothèse* and *La valeur de la Science* point to the following conclusions: (1) none of the physical sciences is purely experimental; each involves logical processes; (2) hypothesis plays an essential rôle in physical science; (3) the principles of the physical sciences are hypotheses; (4) physical theories teach us something of an ultimate objective reality. *Étude sur les théories de la connaissance. IV. Le pragmatisme de l'école française* (pp. 393–422): P. CHARLES. – In the first place an amplification and application, taken largely from *L'évolution créatrice* and *Les données immédiates de la conscience,* of Bergson's statement that our thought is essentially practical; secondly, a criticism of this position in that it involves an unjustifiable separation of the *true* and the useful or practical: "the useful and the true are not opposed the one to the other. As a general rule, we reach the useful through the true." *Analyses et comptes rendus:* G. Rodrigues, *Le problème de l'action:* E. BARON. W. James, *The Meaning of Truth:* E. BARON. J. Zaragueta, *Introduccion general a la Filosofia:* F. CHOVET. H. Schloess, *Introduction à l'étude des maladies mentales:* R. VAN DER ELST-GAUME. E. Picard, *Le droit pur:* C. BOUCARD. Y. Delage et Goldsmith, *Les théories de l'évolution:* F. CHOVET. N. Moeller, *De Leibniz à Hégel:* T. DE VISAN. *Recension des revues et chroniques.*

REVUE DE PHILOSOPHIE. May, 1910. *Quelques problèmes de Logique et d'Histoire de la logique. I. Kant* (pp. 449–466): A. MÜLLER. – In the Kantian philosophy experience is a synthesis of subjective and objective factors. This statement is substantiated by a study of Kant's theory of the *a priori* and his theory of the categories. *L'Absolu, Étude historique (3e Article)* (pp. 467–495): CH. HUIT. – Discussion of the idea of the absolute as found in the various philosophies, from that of Bacon to that of Paul Janet and E. Caro. *Revue critique de morale* (pp. 496–

523): G. MICHELET. – Critical reviews of six or eight recent French works dealing with ethical problems. *Le Devoir est-il une superstition?* (pp. 524–535): X. MOISANT. – Contemporary philosophic thought, contemporary customary thought, and logic itself conspire to answer this question in the negative. *Analyses et comptes rendus:* F. Leenhardt, *L'evolution, doctrine de liberté:* F. CHOVET. Dr. P. Carus, *Philosophy as a science:* F. CHOVET. J. Huré, *Les Assises sociales universelles:* T. DE VISAN. G. Frommel, *La Verité humaine.* W. Ostwald, *L'Energie.* J. Charmont, *La Renaissance de droit naturel:* C. BOUCAUD. J. M. Geonach, *La théorie des idées dans la philosophie de Malebranche:* J. BRUNEL. J. Kremer, *Das Problem der Theodicee in der Philosophie und Literatur des xviii Jahrhunderts.* F. Tocco, *Studi Kantiani. Notes bibliographiques. Recension des Revues.*

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NOTES AND NEWS

WILLIAM JAMES died at his summer home at Chocorua, New Hampshire, on August 26. He had returned from Europe only a week before and his death was quite unexpected. He was born January 11, 1842, and his early studies were scientific in character. His experience with the Agassiz expedition to Brazil in 1865 and his subsequent work at the Harvard Medical School, from which he graduated in 1870, had, no doubt, much to do with the fine breadth of interest and great human sympathy with which he approached all philosophical questions. Professor James lectured on Comparative Anatomy and Physiology from 1872–1880; from 1880 until the close of his university career he occupied himself with psychology and philosophy. His "Principles of Psychology" appeared in 1890. This together with "The Varieties of Religious Experience" (1902) and "Pragmatism" (1907) are the chief landmarks of his influence.

IT is announced that the seventh international congress for criminal anthropology will be held at Cologne in October, 1911.

AT the meeting of the Association of German Scientific Men and Physicians, to be held at Königsberg beginning on September 18, the addresses at the general sessions are as follows: "Epistemology and Science," Professor Külpe, of Bonn; "Puberty and the School," Professor Craemer, of Göttingen; "The Localization of Brain Function," Professor von Monakow, of Zürich; "The Attitude of the Newer Physics to the Mechanical View of Nature," Professor Planck, of Berlin. In connection with the meeting there will be an excursion, starting from Swinemünde on September 5, going on to Wisby, Stockholm, Helsingfors, Wyborg, St. Petersburg and Riga, and ending at Pillau on September 18.

THE Rev. Robert Harley, F.R.S., a congregational clergyman, well known for his important contributions to mathematics and symbolic logic, died on July 26, in his eighty-third year.